with an alternative: either the primordial matter was in a state of homogeneity, and so "infinitely incapable of change," or the homogeneity was disturbed by some external force. But an outside influence is not in the philosophic system. "The admission of inability to evolve the universe without it is an admission that the mechanical philosophy fails at the outset. Nor can it serve the emergency to invoke 'force.' A Divine origin of the universe is usually rejected, because the Divinity eludes the methods of science. But we cannot supplant the Divinity by enthroning force. Science can tell us what force *does*, but it can no more find out what force *is* than what an infinite mind is. Force is an irresistible mental inference from matter in motion, but its ultimate nature is defiantly beyond the reach of science."

The phenomena of life, as exhibited in one of the lower and more minute organisms, are then considered. These are "free and self-originating action"; multiplication; and cyclic change in each new organism. Tiny and humble in organization as these creatures are, they differ vastly from chemical compounds of any kind. The force which animates them differs widely from any mode of force which we call physical. So far as we at present know, the break between "life and not life" is abrupt. Hence, whether or not in the remote past the transition from the one to the other may have been what we should call continuous, our present knowledge offers no explanation of it, and the fact is a stumbling-block in the way of a purely mechanical philosophy.

The remainder of the essay is chiefly devoted to a discussion of the theological aspect of the theory of evolution. This, as designed for the non-scientific part of his audience, need not be further mentioned in these pages. It will be enough to say that, as is now generally admitted by the more intelligent among theologians, he maintains that there is no necessary antagonism between their beliefs and scientific theories.

As might be expected from him, Dr. Dallinger is temperate in expression and eloquent in language. Some readers perhaps would have preferred a little more conciseness in style and statement, but it must be borne in mind that the discourse was delivered as a lecture to a non-scientific audience, who required leading gradually, or even alluring, into unfamiliar paths of thought. Among such persons the book cannot fail to do excellent work in allaying needless fear and silencing ignorant clamour; among opponents it will serve to show that the Theist's position is more defensible than they suppose, and that, in their own, unsuspected difficulties lurk beside the seemingly easy path of a euphonious terminology.

OUR BOOK SHELF.

The Harpur Euclid. Book I. By E. M. Langley and W. S. Phillips. (Rivingtons, 1888.)

THE editors are mathematical masters of two Bedford schools under the Harpur Trust; hence the title. For the work itself the title-page further informs us that it is an edition of Euclid's "Elements" revised in accordance with the Reports of the Cambridge Board of Mathematical Studies, and the Oxford Board of the Faculty of Natural Science. Extracts from these Reports are given in a prefatory note: this is the only part of the work which is not strictly adapted for the use of school-boys.

We began our task with no special liking for it, but had not proceeded far when we found that there were new adornments which rendered our perusal of the familiar lines very agreeable. We read on through 102 out of the 120 pages without break, and then ceased, as we had come to some matters which required more careful examination. The editors have kept to the usual sequence, but in many cases have replaced the Simsonian demonstrations by easier ones, and have discarded much of the superfluous matter which has led anti-Euclidians to inveigh so strongly against the "Elements."

We are glad to see that exercises come in right from the outset; these all seem to have been most carefully selected, and are such as a fairly intelligent boy ought to be able to solve from the previous propositions. We refer here to the examples in the body of the book. Frequent reference is made to that excellent, though perhaps hardly sufficiently appreciated, little book of Prof. Henrici, "Congruent Figures," and to the "Syllabus" of the Association for the Improvement of Geometrical Teaching. At the end, as a kind of appendix, are some judicious sections on properties of triangles, on quadrilaterals, on loci, on solving geometrical problems—(1) method of intersection of loci; (2) method of intersection of sets; (3) method of analysis and synthesis. Considerable pains has been bestowed on the arrangement of the text, the selection of the various types, and the drawing of the figures; in fact, the little book is the beau-ideal of a Euclid for boys. We wish we had had such a book in the "auld lang syne," and then our first perusal would not have been so painful. It is the authors' intention to bring out the successive books in like form. We wish them like success, and trust that their venture will find a welcome in many a school.

A Course of Quantitative Analysis for Students. By W. N. Hartley, F.R.S. (London: Macmillan and Co., 1887.)

AFTER the almost infinite number of books, mostly small, "and mostly to meet certain requirements of our own students" on qualitative analysis, it is a relief to meet with a small book for students—beginners—on quantitative analysis, written evidently for beginners, and in a manner to really lead them up from qualitative notions, not by one great bound, but by good sober practice and order, to the appreciation of the care and exactitude, and most important still, the "criticising" state of mind necessary to make a real analytical chemist.

As the author says in his preface: "To be a good analyst does not necessitate a profound knowledge of chemistry;" but any student who has worked at all well through this little book will have a good platform of knowledge under him, and be in a position to enlarge his knowledge with infinitely greater ease, and that very necessary regard for accuracy which is not possible to a student who has not done any quantitative work.

The author begins in a sensible manner by giving the metric weights and measures, with English equivalents, and then the dimensions of various laboratory apparatus, beakers, &c., and all this is very useful. In the introduction, manipulation and reagents are dealt with. The author might have added the use of folded or plaited filters. It is quite as safe and accurate to use them for quantitative purposes as to employ a pump.

Before proceeding to simple estimations of constituents of salts, &c., we have about twenty pages of introductory examples devised with the intention of enabling students to realize the meaning of the atomic and equivalent weights of elements; which they do not always do when put on to determinations without any introduction. This is the most useful and original part of the book. The following exercises, "simple estimations," are fairly in order of difficulty. The middle portion of the book is on volumetric analysis. It is short but workable, and is followed

© 1888 Nature Publishing Group

by a good section on analysis of silicates and some technical products. The book does not attempt to cover all the field of analysis, but what is done will be found really useful by a beginner or a junior student.

W. R. H.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*

"A Conspiracy of Silence."

THE Duke of Argyll can scarcely be congratulated upon his latest discovery of a new ground of attack upon geologists. In the year 1862 a very eminent physicist, whose loss we all so deeply deplore, made the somewhat rash suggestion that flint implements are found deep down in the drift, owing to their high density as compared with that of the matrix in which they are inclosed. Seeing that the material in which the implements are found is usually a *flint*-gravel, everyone acquainted with the subject saw that the suggestion was, to say the least, a somewhat unfortunate one, and Prof. P. G. Tait, in seeking for an opportunity to sneer at "advanced geologists," was scarcely kind to the memory of a deceased friend in rescuing such a suggestion from oblivion. But to the Duke of Argyll, the finding of a new basis from which to attack geologists seems to have been a chance which he could not afford to let slip.

The Duke of Argyll now asks when we are going to begin to discuss his magazine-article upon coral reefs. I reply that in the article in question there is not a single new fact or fresh argument—nothing which has not been already brought forward by Mr. Murray himself, or by Dr. Archibald Geikie, and met by Prof. Dana in a singularly exhaustive memoir well known to all geologists. The subject has, moreover, been treated at considerable length by Profs. Prestwich, Green, James Geikie, De Lapparent, and others. Surely no exception can be taken either to the eminence of the authorities who have written on the subject, to the length to which their notices have extended, or to the prominence of the journals or treatises in which these discussions have appeared. If it be said that the general scientific public have not had the matter fully laid before them, it is only necessary in reply to call attention to the pages of NATURE, in which a succession of articles dealing with the subject will be found.

The Duke of Argyll says that he has "nothing to retract." Here I regret to have distinctly to join issue with him. He has asserted that scientific men have refrained from discussing a particular theory, and that in taking this course they have been actuated by the worst of motives—a fear of the truth; he has charged the Geological Society with refusing in the spring of 1885, through its then President, to accept a certain paper from the same cause; and now he adopts and gives fresh currency to an equally offensive charge of a similar kind.

These charges have, each and all of them, been shown to be absolutely destitute of foundation. The Duke of Argyll must judge for himself if the principle of *noblesse oblige* should not lead him, not only to *retract* the charges, but also to *apologize* for having made them. But his Grace may rest assured that, until he does so, the grounds for the deep indignation at his conduct, which is so strongly felt both at home and abroad, will still remain.

JOHN W. JUDD.

On the Constant P in Observations of Terrestrial Magnetism.

I REGRET that Prof. Rücker should have largely misunderstood my last letter. I have not raised the question of fallible observations at all. Referring to the correspondence on pages 127–8 of the present volume of NATURE, my principal contention was and is that the ordinarily accepted formula for P differs by terms of the second and higher orders from Gauss's theory, and that that difference necessarily persists in any rigorous expansion of the formula. By the ordinarily accepted formula for P I mean Prof. Rücker's formula (α); and by Gauss's theory I mean my formulæ (1), (2), and (3). From two observations of $f(u)$, made respectively at the distances r and r_1, the L of Gauss's theory might be found by a direct solution of equations (1) and (2); but instead of that, it is customary to find L from equations (7) and (8) by substituting in them the value of P_0 computed through equation (α). To render the latter procedure rigorous, P should be used in (7), and P_1 in (8). Equation (11) shows that P and P_1 differ by quantities of the second and higher orders, and as the ordinarily accepted value of P_0 lies between P and P_1, it necessarily differs from one or both of these quantities, and therefore from Gauss's theory, by terms of the second and higher orders.

While freely admitting the justice of Prof. Rücker's criticism upon my arbitrary assumption that $P_0 = \frac{1}{2}(P + P_1)$, I cannot assent to the process by which he has deduced equation (γ). Equations (7) and (8) show that we may have either one L and two P's, or two L's and one P. In the latter case these equations become—

$$\tfrac{1}{2}L' = A(1 - P_0 r^{-2}) \quad . \; . \; . \; . \; . \; . \; (15)$$

$$\tfrac{1}{2}L'' = A_1(1 - P_0 r_1^{-2}) \quad . \; . \; . \; . \; . \; . \; (16)$$

and P_0 must be determined so as to make L′ and L″ as nearly as possible identical with L. To that end we must have $2L = L' + L''$; and then, from the difference between (7) + (8) and (15) + (16)

$$P_0 = B(A - A_1)\frac{r_1^2 + r^2}{Ar_1^2 + A_1 r^2} \quad . \; . \; . \; . \; . \; (17)$$

Expanding to terms of the second order

$$P_0 = B\frac{(A - A_1)}{A}\left\{1 + \frac{r^2}{r_1^2 + r^2}\left(\frac{A - A_1}{A}\right)\right\} \quad . \; (18)$$

Whence, by equation (13)

$$P_0 = \frac{r_1^2 r^2}{r_1^2 - r^2}\left(\frac{\log A - \log A_1}{M}\right) - \frac{r_1^2 r^2}{2(r_1^2 - r^2)}\left(\frac{\log A - \log A_1}{M}\right)^2 \quad . \; . \; (19)$$

This result agrees better with equation (14) than with equation (γ).

WM. HARKNESS.

Washington, D.C., December 30, 1887.

I AM afraid that the new method of calculating P_0 adopted by Prof. Harkness is not less arbitrary than that which he previously employed. He says that "P_0 must be determined so as to make L′ and L″ as nearly as possible identical with L." If the object is only to deduce a correct value of L by combining equations (15) and (16), this condition is certainly not necessary. For if we substitute from (17) in (15) and (16), and take the mean of the values of L′ and L″, we get by a very roundabout process the same value of L as we should have obtained without using P_0 at all. But we should have reached the same final result if we had started with the assumption that

$$(n + m)\,L = n\,L' + m\,L'',$$

where n and m are any numbers whatever. By properly choosing n and m we could deduce the correct value of L with any assigned value of P_0. It appears to me that the equation $2L = L' + L''$ is based upon the tacit assumption that L′ and L″ are to be combined in accordance with the rules applied to fallible measures, and cannot otherwise be justified if the only object is the correct deduction of L from (15) and (16).

If, however, P_0 is introduced to enable us to calculate another approximate value of L by observing (say) A_2 at some other distance, r_2, the best value to select will depend on circumstances. If r_2 is nearly $= r$ we shall get the best result by writing $P_0 = P$ and so on, so that the equation $2L = L' + L''$ is again arbitrary.

I am quite in agreement with Prof. Harkness as to the fact that if we start from the basis of equations (1) and (2) a small theoretical error is introduced by substituting P_0 for P and P_1. Indeed I think this step can only be justified by our knowledge that the inaccuracy thus caused is less than the error of experi-

© 1888 Nature Publishing Group